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International travel in times of the COVID-19 pandemic: The case of German school breaks☆

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ABSTRACT

The COVID-19 pandemic has triggered severe global restrictions on international travel with the intention of limiting the spread of SARS-CoV-2 across countries. This paper studies the causal effect of the partial relaxation of these travel restrictions in Europe on the COVID-19 incidence in Germany during the summer months of 2020. It exploits the staggered start of the summer school breaks across German states as an exogenous shock to the travel opportunities of the population. While the school breaks also increased mobility within Germany, the event-study type regressions precisely control for domestic mobility and local COVID-19-related restrictions. The intention-to-treat effects of the relaxed travel restrictions show a significant and sizable increase of the COVID-19 incidence in German counties during the later weeks of the school breaks. Part of the increase can be attributed to a mandatory testing regime for travel returnees from high-incidence areas.

1. Introduction

Following the outbreak of the SARS-CoV-2 pandemic, Germany implemented a number of non-pharmaceutical interventions (NPIs) to slow down the spread of the virus and to prevent the German health care system from being overwhelmed. After a steep increase in infections in March and a peak in April of 2020, the number of new confirmed cases of COVID-19 infections in Germany dropped sharply in the subsequent weeks, similar to the patterns observed in other European countries.

The restrictions on cross-border movements and international travel constituted one of the most drastic and unprecedented NPIs, as they brought travel both outside and within Europe largely to a hold during the early months of the pandemic. However, Germany's restrictions on intra-EU travel were considerably relaxed on June 15 (Deutsche Welle, 2020c), as indicated by the vertical line marking calendar week 25 in Panel 1a of Fig. 1, closely followed or preceded by other EU countries. Three weeks later, the weekly incidence of COVID-19 in Germany began to increase. Over the course of the following six weeks, it more than doubled, continuing its ascent after a brief plateau between calendar weeks 34 and 36.

This paper examines the causal relationship between the resurgence of international travel and the COVID-19 incidence in Germany. The empirical strategy exploits that the relaxation of the travel restrictions closely coincided with the beginning of the summer breaks in German schools. While the breaks generally last six weeks in all 16 German states, their timing is staggered across the summer months. The first states went on school breaks in calendar week 26 and concluded them by week 32, while the latest states began their school breaks in calendar week 31 and concluded them not before week 36. This period is indicated by the two vertical gray lines in Panel 1b of Fig. 1. The timing of the school breaks has further not been changed due to the pandemic. The exogenous and staggered timing of the school breaks therefore provides an ideal setting for an event study approach; it is also used by Isphording et al. (2021) and von Bismarck-Osten et al. (2020) to evaluate the effect of school closures and reopenings on the COVID-19 incidence in Germany. In the context of this study, the staggered school breaks represent an exogenous shock to the probability that individuals and families with school-aged children residing in a specific German state will travel during the summer months.

The hypothesis that international travel may have contributed to the rising COVID-19 incidence in Germany is motivated by the fact that residents of Germany traveled to other European countries that exhibited a considerably higher COVID-19 incidence during the summer months of 2020 than Germany. Hence, at least at the national level,

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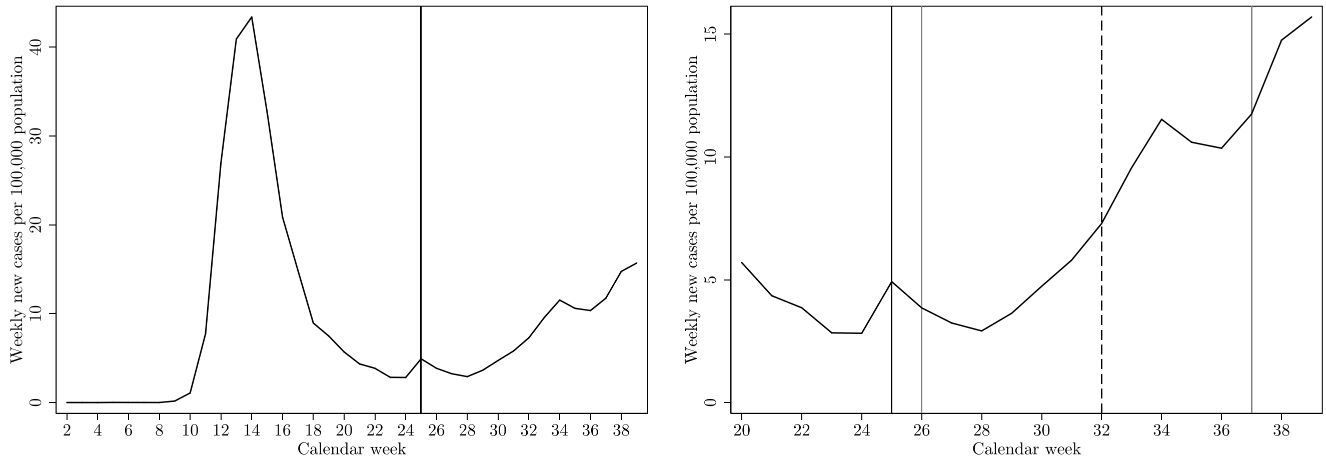


(a) Incidence of COVID-19 in Germany (b) New COVID-19 cases per 100,000 population

Fig. 1. Incidence of COVID-19 and school breaks in Germany in 2020. Notes: The left panel shows the weekly number of new confirmed cases of COVID-19 infections per 100,000 population between calendar weeks 2 and 39 in Germany. The vertical black line at week 25 indicates the relaxation of international travel restrictions. The right panel shows the weekly number of new confirmed cases of COVID-19 infections per 100,000 population between calendar weeks 20 and 39 in Germany. The vertical gray lines at week 26 and week 37 indicate the earliest beginning and the latest conclusion of the school breaks in German states. The vertical dashed black line at week 32 indicates the beginning of a mandatory testing regime for returning travelers from risk areas as declared by the RKI.
Source: Author's own depiction based on data by RKI (2020c).

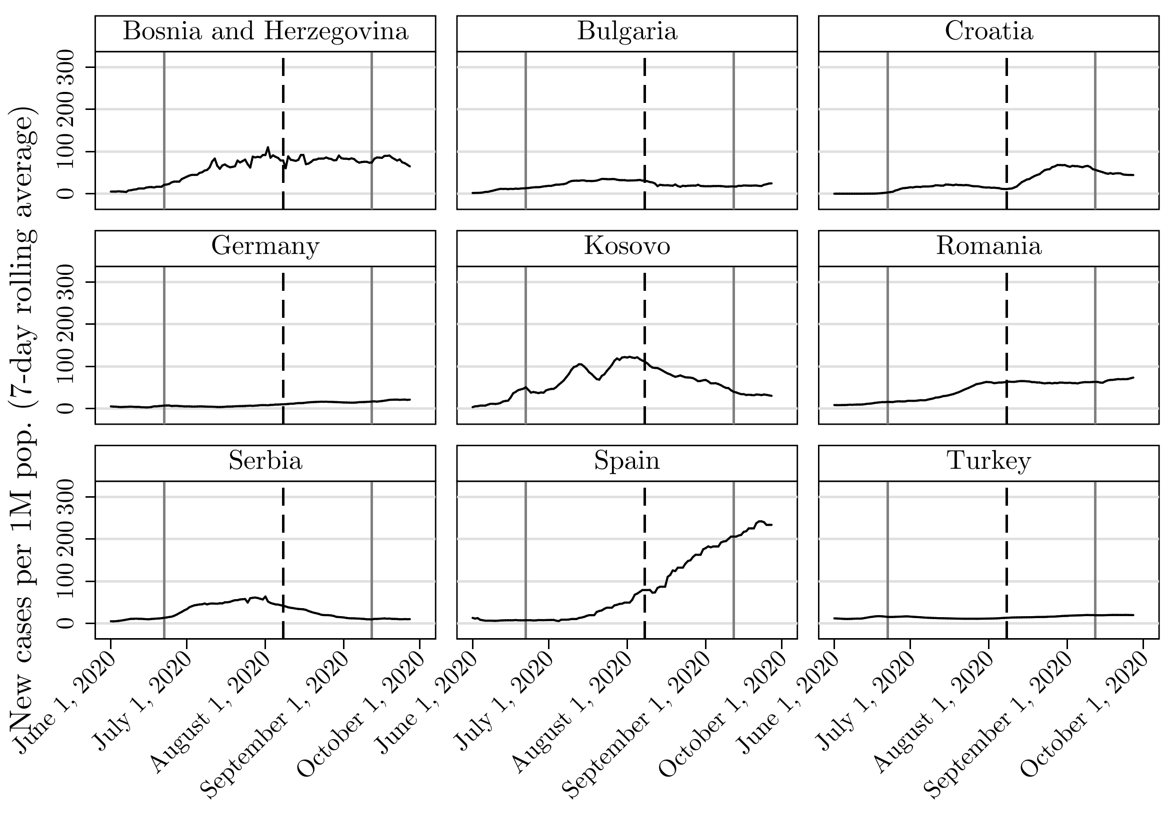
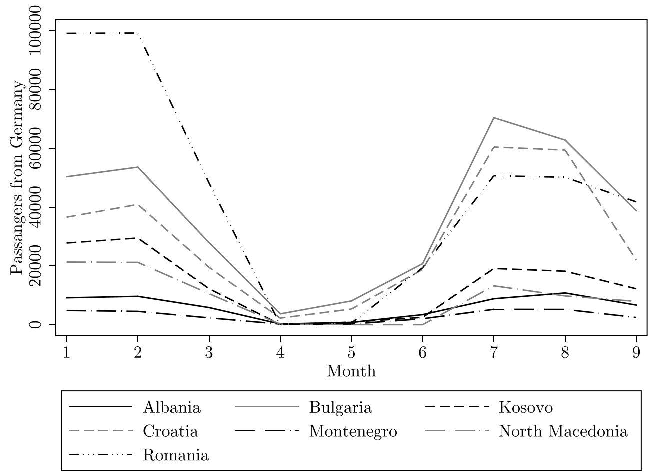


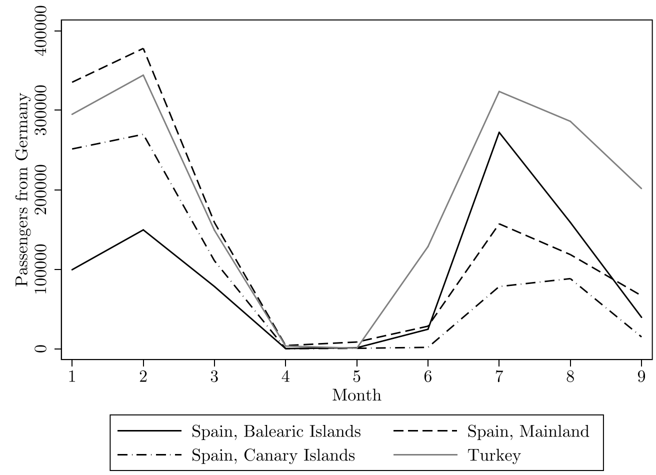
Fig. 2. Cases in Germany and other European countries during the summer months of 2020. Notes: The gray vertical lines in each graph indicate the start of the earliest school break and the end of the latest school break respectively in Germany in 2020. The black dashed line in each graph indicates the implementation of the mandatory testing regime for returning travelers from risk areas.
Source: Author's own depiction using data by Roser et al. (2020).

these travelers were exposed to environments that carried a relatively higher risk of infection, implying the potential for importing infections into Germany. The population-adjusted incidence of COVID-19 in several popular European summer travel destinations in comparison to the incidence in Germany is displayed in Fig. 2, with the gray vertical lines indicating the total duration of all school breaks in Germany. Most of the displayed countries exhibited a higher incidence than Germany, not even attempting to take into account that surveillance of COVID-19 may have been more constrained in some of these countries.

The results of this study indicate a statistically significant increase in



(a) To Southeast European countries



(b) To Spain and Turkey

Fig. 3. Passengers departing from German airports in the first nine months of 2020. Notes: Panel 3a displays the number of passengers departing from airports in Germany to a number of Southeast European countries during the first nine months of 2020. Panel 3b displays the number of passengers departing from airports in Germany to Turkey and several regions of Spain during the first nine months of 2020.
Source: Author's own depiction based on data provided by Destatis (2020b).

COVID-19 incidence in German counties in the second half of the school breaks. This pattern is robust to the inclusion of disaggregated and time-varying controls for mobility and COVID-19 restrictions within Germany. Magnitude and dynamics of the event study estimates are consistent with descriptive statistics on infections detected among travelers returning from abroad during the school breaks.

The paper contributes to the evidence on the effectiveness of NPIs in containing the COVID-19 pandemic. A major advantage over several other studies is that the timing of the school breaks had been exogenously determined before the beginning of the pandemic. This feature diminishes the threat of reverse causality, which potentially affects the validity of studies examining the introduction of NPIs such as mandatory face mask mandates and stay-at-home orders, as NPIs are typically introduced when the epidemiological situation is demanding them (Backhaus, 2020). While overall, the relaxation of the travel restrictions was possible because of the relatively low incidence at that time, the particular shock to the probability of a state's population to travel arrived exogenously via the staggered school breaks. A notable limitation of this paper, in turn, is that actual travel is unobserved both at the individual level and at higher levels of aggregation such as counties. The results should therefore be interpreted as intention-to-treat effects (ITT): Not every resident in Germany experienced a shock to her/his travel opportunities from the combination of the relaxed restrictions and the school breaks - and among those who have, not everyone has actually traveled.

2. Literature review

Several studies have documented the role of international travel in the early spread of SARS-CoV-2 across countries (Zhang et al., 2020; Murphy et al., 2020; Böhmer, 2020; Rothe et al., 2020), with Hodcroft et al. (2021) demonstrating the rapid spread of a SARS-CoV-2 variant from Spain to other European countries during the summer of 2020.

A number of modeling studies have then attempted to assess the effectiveness of travel restrictions and border closures on the spread of the virus, e.g. Brady et al. (2020); Chinazzi et al. (2020); Wells et al. (2020); Costantino et al. (2020); Wu et al. (2020); Linka et al. (2020); Ruktanonchai et al. (2020); Russell et al. (2020). Another strand of the literature has estimated the effectiveness of border closures and travel restrictions without relying on epidemiological models (Koh et al., 2020; Kraemer et al., 2020; Keita, 2020; Eckardt et al., 2020). Typically, these studies examine a bundle of NPIs enacted during the early stage of the pandemic. However, the close succession of NPIs and the uncertainty surrounding the accurate surveillance of SARS-CoV-2 in this period present two challenges to their approach. Further, these studies typically cannot address the potential reverse causality between the enactment of NPIs and the epidemiological situation.

A meta-review on travel-related control measures highlights the imbalance between modeling studies and observational studies, assessing a lack of 'real-life' evidence on the effectiveness of these measures (Burns, 2020). The certainty of the evidence for most travel-related control measures was rated as low, due to inappropriate assumptions in the modeling studies on the one hand and potential bias in the observational studies on the other hand. Hence, it is reasonable to complement the existing literature with evidence based on exogenous variation in travel opportunities.

3. Context

3.1. Restrictions on international travel during the early stage of the pandemic

Restrictions on international travel were imposed globally in the early months of the pandemic in order to prevent and limit the transmissions of the virus across national borders. From a European perspective, the restrictions affected travel both between EU countries on the one hand and between the EU, other European countries, and the rest of the world on the other hand. In mid-March 2020, the EU closed its external borders for travelers from non-EU countries (Deutsche Welle, 2020b). Simultaneously, the EU member states put in place restrictions on non-essential travel of EU citizens across their national borders, with limited exceptions. The EU member states differed in the detailed design of the travel restrictions applying to their respective territories. Germany issued a global travel warning in mid-March, advising its citizens against any travel abroad, while imposing border controls at its borders to neighboring countries (Deutsche Welle, 2020a).

De facto, the coordinated actions taken by the EU member states severely halted both mobility within the EU and mobility between the EU and the rest of the world. The travel restrictions remained in place till the summer months regarding travel within Europe. By mid-June 2020

Table 1
Number of infected travel returnees by country of likely infection and calendar week in 2020.

Week	Kosovo	Croatia	Turkey	Romania	Spain	All five countries
27–30	303	29	70	36	17	455
31	341	45	123	40	27	576
32	564	235	393	56	76	1324
33	847	588	670	111	120	2336
34	958	1153	496	174	296	3077
35	767	895	363	208	211	2444
36	426	638	403	208	140	1815
37	168	208	346	165	103	990
38	91	90	261	103	60	605
39	44	43	156	84	46	373
Total	4509	3924	3281	1185	1096	13995

Source: Author's own calculation based on various RKI situation reports.

Table 2
Start and end dates of summer school breaks in German states in 2020.

State	Start date	End date	Start week	End week
Mecklenburg-Western Pomerania	22.06.	01.08.	26	31
Hamburg	25.06.	05.08.	26	31
Berlin	25.06.	07.08.	26	32
Brandenburg	25.06.	08.08.	26	32
Schleswig-Holstein	29.06.	08.08.	27	32
North Rhine-Westphalia	29.06.	11.08.	27	32
Hesse	06.07.	14.08.	28	33
Rhineland-Palatinate	06.07.	14.08.	28	33
Saarland	06.07.	14.08.	28	33
Bremen	16.07.	26.08.	29	34
Lower Saxony	16.07.	26.08.	29	34
Saxony-Anhalt	16.07.	26.08.	29	34
Saxony	20.07.	28.08.	30	35
Thuringia	20.07.	29.08.	30	35
Bavaria	27.07.	07.09.	31	36
Baden-Württemberg	30.07.	12.09.	31	37

Source: Author's own compilation.

then, the EU made coordinated efforts to revive travel between its member states and nearby countries (Deutsche Welle, 2020c). Complementary, the German government revoked its travel warnings for most of the other EU countries. The sharp drop in international travel and its swift recovery are reflected in Fig. 3 displaying the number of passengers departing from Germany to states in Southeast Europe, Turkey, and Spain over the first nine months of 2020. In general, transportation statistics suggest that the revived air travel from and to Germany remained largely focused on Europe during the summer months: More than 90% of passengers who departed from German airports during the four months from June to September 2020 had other European countries as their flights' countries of destination; similarly, more than 90% of passengers arriving at German airports during this period arrived from other European countries (Destatis, 2020b,c). These shares increased by at least ten percentage points in comparison to the average of the years 2015–2019. Both the total numbers of passengers departing and arriving in the months June-September 2020 were down by more than 80% compared to the 2015–2019 average.

The German federal government maintained surveillance of the epidemiological situation abroad via the RKI (Robert Koch Institute, Germany's federal disease control and prevention agency). The RKI would issue new travel warnings and designate foreign countries or regions as 'risk areas' by following a procedure which was primarily though not exclusively oriented towards the 7-day-incidence of COVID-19 cases abroad (RKI, 2021b). Consequently, travel warnings were issued for a number of European countries and for most countries in the rest of the world simultaneously with the general relaxation of the travel restrictions.

However, while travel remained largely restricted to Europe, the travel warnings maintained or reissued by the German federal government do not appear to have regulated travel between Germany and other European countries during the summer months of 2020. For example, the travel warnings for the Southeast European states of Kosovo and Serbia were never rescinded during this period; nonetheless, more than 95,000 passengers departed from German airports towards these two countries between June and September 2020 (RKI, 2020b; Destatis, 2020b).

3.2. Epidemiological situation of travel returnees

Over the course of the summer months, evidence began to accumulate at local public health offices and the RKI that a growing number of infections confirmed by tests in Germany had been contracted abroad. This surge in cases with probable infection abroad was partly accompanied by the introduction of a free testing regime for all returning travelers from non-risk areas on August 1 and a mandatory free testing regime for travelers returning from the designated risk areas on August 8. While travelers returning from risk areas had already been required to quarantine for 14 days in their residences in accordance with regulations passed by the German states in mid-July, enforcement of the quarantine was in the hands of the local public health offices, relying at least partly on voluntary compliance. The free and voluntary tests for returnees in general and the free and mandatory tests for returnees from risk areas were hence suitable for improving the surveillance of infections among returnees. While the free voluntary testing regime was terminated on September 15, three days after the last state had completed its school breaks, the mandatory testing regime remained in place. (Deutsche Welle, 2020d) The potential impact of the two testing regimes on the estimation results of this study is discussed in Section 6.3.

In total, the RKI registered more than 24,000 confirmed cases with likely place of infection abroad among returning travelers between the calendar weeks 26–39 (06/22/2020–09/27/2020) (RKI, 2020a). This figure provides a useful orientation regarding the number of infections introduced into Germany from returning travelers during the summer months. Table 1 highlights that only five travel destination countries account for almost 14,000 of the 24,000 confirmed cases among travel returnees, with four of the five countries being located in Southeast Europe.

However, not all cases of infected travel returnees detected in Germany may have been correctly classified as having their origin of infection abroad. Further, infected travel returnees may have caused secondary infections and subsequently detected cases among their contacts back in Germany. In addition, travel to destinations abroad had already been picking up pace since June, as indicated in Section 3.1, and by early August, all states had already begun their school breaks, with some concluding them shortly before or around the time when the free and mandatory testing regimes were implemented. Taken together, there is reason to suspect that the public surveillance of the travel returnees may not have fully captured the contribution of international travel to the epidemiological situation in Germany during the summer months of 2020.

3.3. Summer school breaks in Germany

The travel restrictions were relaxed simultaneously all across Germany, thereby not providing a control group of German locations where the restrictions were still in place. Therefore, this paper exploits the staggered timing of summer school breaks across Germany's 16 federal states as an exogenous shock to the opportunities of the population to embark on international travel. While the summer break lasts six weeks in every state, the start and end dates of the break generally differ in each year depending on the state of residency. The start and end dates for the 2020 school summer vacations were set before the beginning of the pandemic; they have not been altered since then. The earliest state to begin the summer break was Mecklenburg-Western Pomerania on June 22 in calendar week 26, shortly after the suspension of the travel warnings, while the latest state to conclude the summer break was Baden-Württemberg on September 12 in calendar week 37. Hence, schools have been on the six-week summer break in at least one German state over a total period of twelve consecutive weeks. All start and end dates in the respective states are displayed in Table 2.

It is reasonable to assume that the duration of the school breaks served as a stern constraint on the ability of families with school-aged children to travel. While schools had been closed during the initial months of the pandemic in Germany, the states had returned to at least partial or rotating in-class instruction before the start of the school breaks. Further, return to in-class instruction had been announced and was henceforth expected after the summer break. Therefore, travel with school-aged children could not be delayed until after the end of the school breaks. Finally, even if significant travel with school-aged children already occurred before the official start of the school breaks, this would bias the estimate of the school break effects towards the null, as travel and potential imported cases would occur earlier than the official dates of the school vacations would suggest.

Regarding the relative size of the population whose travel opportunities were affected by the school breaks, families with underaged children represent at least 30% of the population in every German state, as displayed in Table A.1 in the Appendix. The mobility potential of the population in question is therefore sufficiently large to affect the general population incidence.

4. Empirical strategy

The empirical investigation into whether international travel has increased the COVID-19 incidence in Germany is complicated by the fact that the travel restrictions were relaxed simultaneously all across Germany, thereby not providing a control group of German locations where the restrictions were still in place. However, while the international travel restrictions were eased simultaneously all over Germany, the staggered starting dates of the school breaks across states provide exogenous variation in the opportunities to travel, primarily for families with school-aged children. At the beginning of the observation period, all units are untreated, meaning no state is on school break yet. Over time, more and more states start their school breaks and are hence treated. After a certain date, all states have begun their school breaks, which corresponds to the treatment being switched on for all units in the sample. The setting of the German summer school breaks therefore presents the opportunity for an event study approach.

The event study design estimates dynamic treatment effects of an event, in this case the school breaks, on a given outcome, here the COVID-19 incidence. Due to their dynamic nature, these treatment effects are allowed to vary over time. Hence, an individual effect is estimated for each period in a given time window around the event. By contrast, the canonical difference-in-differences (DiD) framework would yield only a single estimate of the (presumably constant) treatment effect. However, in a setting with staggered treatment, dynamic treatment effects, and two-way fixed effects, the DiD estimate is biased towards zero (Goodman-Bacon and Marcus, 2020; Goodman-Bacon, 2021). The event study design is particularly convenient for the case of international travel following the start of the school breaks, as it is not a priori obvious when the treatment could show an effect: Following the beginning of the school breaks, people have to travel, they have to get infected, and they have to return while still infected or having overcome their infection only recently in order to test positive upon or after arrival back in Germany.

The exogenous timing of the school breaks further alleviates concerns in many other studies evaluating the effects of NPIs, namely that a specific restriction or a package thereof would be introduced (or lifted) more likely in places that necessitate (or allow) it. While overall, the lifting of travel restrictions was certainly encouraged by the low COVID-19 incidence in early June, the beginning and the end of the school breaks in the various states were unaffected by the epidemiological situation.

A crucial assumption in the context of DiD and event study settings is the parallel trends assumption (PTA), which states that the untreated or, given that all units are eventually treated in this case, the not-yet-treated units follow the same trend that the treated units would have followed had they not been treated. Under the PTA, the not-yet-treated units provide a valid counterfactual to the treated units. In turn, a violation of the PTA would call into question any causal interpretation of the DiD and event study estimates, as the latter might simply be the result of diverging trends between treated and not-yet-treated units. In the context of event study designs, the potential for the PTA to hold is usually assessed by examining the event study estimates for pre-treatment periods. If these estimates are statistically insignificant, this may indicate that the PTA is not violated at least prior to the treatment.

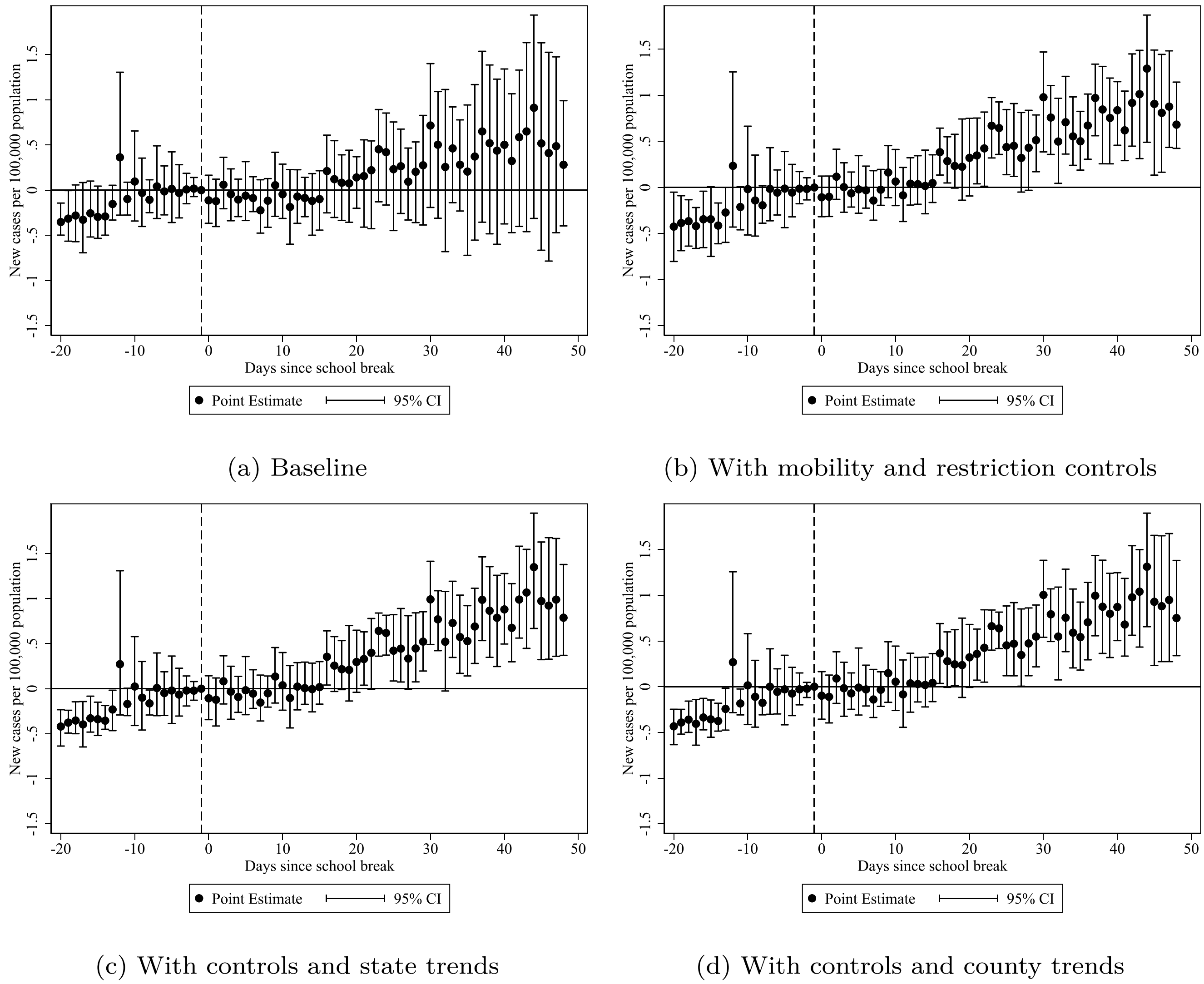
A related assumption is that the treatment is not anticipated by the not-yet-treated units, such that the estimates from the pre-treatment periods do not already reflect effects of the treatment, for example due to the not-yet-treated units already adapting their behavior in anticipation of the treatment. Regarding the population with school-aged children, such anticipation effects can virtually be ruled out due to the binding character of in-person instruction until the school breaks. Regarding the population without school-aged children, anticipatory behavior, such as traveling prior to the start of the school breaks, would have been hardly possible in those states that went on school breaks first, as the travel restrictions had only been relaxed briefly before the earliest school breaks and on rather short notice. In those states that went on school breaks later, the population without school-aged children might have to some extent anticipated the school breaks by intentionally traveling before their start. However, given that a certain time span is expected to elapse between the outbound travel and the detection of an infection upon return, effects of anticipatory travelers on the COVID-19 incidence should still mostly be picked up by the post-treatment event study estimates and not contaminate the pre-treatment estimates.

A final threat to this identification strategy is the possibility that the school breaks may have increased COVID-19 incidence in Germany not only by inducing more international travel and hence the introduction of infections from abroad, but also by increasing mobility and social contacts within Germany, thus increasing the COVID-19 incidence without any significant contribution from international travel. In addition, COVID-19 restrictions unrelated to international travel have become more scattered across the German states in terms of their strictness as compared to the first nationwide contact restrictions in spring. Therefore, it is indispensable to control for changes in mobility and restrictions over time and at a more disaggregated level. Fortunately, data are available to construct such time-varying controls at the level of German counties (German: *Kreise*).

Given that actual travel is unobserved, the effect of the school breaks on new cases of COVID-19 is estimated by directly regressing the new cases in Germany on the set of event indicators. This procedure yields reduced-form estimates of the effects that the relaxed travel restrictions combined with the school breaks have on the COVID-19 incidence, as opposed to the effects of actual travel on the incidence. The results can further be interpreted as intention-to-treat (ITT) effects, as not every resident in Germany experienced a shock to her/his opportunities to travel from the combination of the relaxed restrictions and the onset of the school breaks - and among those who have, not everyone has actually traveled.

Following the notation proposed by Clarke and Schythe (2020), the regression model for the event study is formulated as follows:

$$y_{st} = \alpha + \sum_{j=2}^{J} \beta_j (\text{Lead}\, j)_{st} + \sum_{k=1}^{K} \gamma_k (\text{Lag}\, k)_{st} + \mu_s + \lambda_t + \theta_v \times \nu_w + X'_{st}\Gamma + \epsilon_{st} \tag{1}$$



(a) Baseline

(b) With mobility and restriction controls

(c) With controls and state trends

(d) With controls and county trends

Fig. 4. New cases of COVID-19 per 100k population during school breaks in Germany. Notes: All panels display the daily new confirmed cases of COVID-19 infections in Germany during the summer school breaks in 2020. The effects are estimated by binning all weeks beyond the maximum Leads and Lags before and since the beginning of the school breaks. Standard errors are computed by the wild clustered bootstrap method and clustered at the state level.
Source: Author's own depiction.

y_{st} is the number of new confirmed cases of COVID-19 per 100,000 population in county s on day t. α is a constant. μ_s and λ_t are county and day fixed effects respectively, while ϵ_{st} is an unobserved error term. θ_v are state fixed effects that are interacted with fixed effects ι_w for the days of week in order to capture differential reporting patterns of the states across the days of the week. X_{st} contains county-level and time-varying controls, which will capture daily patterns of mobility and changes to COVID-19-related restrictions during the observation period. The J leads and K lags are binary variables that indicate that the given state was a given number of periods away from the beginning of the school break in the respective period, with the leads denoting the number of periods prior to the onset of the school breaks and the lags denoting the number of periods since the onset of the school breaks. One period serves as baseline and is hence omitted; this is the last period before the onset of treatment, which corresponds to the first lead.

The main specification uses up to 21 leads and up to 49 lags. The rationale for using up to 21 leads is that this time window should be sufficient for detecting potential violations of the parallel trends assumption in the pre-treatment period, which statistically significant estimates of the β_j coefficients would indicate. In turn, statistically significant estimates of the γ_k coefficients would indicate effects of the treatment, in this case the school breaks, on the COVID-19 incidence. While the school breaks last only six weeks (42 days), the following seven lags are intended to capture potential infections that have taken place in the last days of the school breaks but which have only been confirmed by test results in the course of the next days. All regressions in the following are weighted by county population.

The regression results in this paper and their graphical representations are obtained by applying the user-written Stata routine `eventdd` (Clarke and Schythe, 2021). Among its numerous functionalities, which are presented in more detail by Clarke and Schythe (2020), this routine allows binning lead and lag periods beyond the specified maximum lead and lag periods into the final lead and lag terms, as suggested by Schmidheiny and Siegloch (2019) in settings where all units are eventually treated. This binning procedure is applied in all regressions reported in the following. The routine further provides the option to calculate confidence intervals by applying the wild cluster bootstrap method via the user-written Stata routine `boottest` (Roodman, 2021; Roodman et al., 2019). The wild cluster bootstrap is particularly relevant in the setting of this study: The timing of the summer breaks varies at the level of Germany's states, which suggests clustering the standard errors of the coefficients at the state level. However, there are only 16 states in Germany, which may downward-bias the cluster robust variance estimate (Cameron and Miller, 2015). The wild cluster bootstrap has been shown to provide reliable inference even if the number of clusters is small (Cameron et al., 2008).

5. Data

Data on the COVID-19 incidence in Germany are provided by RKI (2020c). The data are available in daily format and disaggregated to the level of 401 German counties (German: *Kreise*). The daily reported cases are subject to considerable reporting variability across the days of the week. On top of the day and county fixed effects, the interactions of the state and the day-of-the-week fixed effects included in Eq. (1) are intended to capture such variations. Using official population data, the daily incidence of COVID-19 per 100,000 population is computed and used as the dependent variable in the following.

The incidence dataset contains the date (German: *Meldedatum*) when a new confirmed case of COVID-19 was reported to the RKI. The report to the RKI is usually the consequence of a positive result of a PCR test for SARS-CoV-2. Given that Germany has preferably been testing symptomatic individuals, a delay of several days may exist between the infection date and the reporting date to the RKI, with the length of the delay depending on the length of the period between infection and the development of symptoms on the one hand, and on the length of the period between taking a test and receiving the result on the other hand. Given the rather large uncertainty surrounding this delay, the regressions reported in the following do not make any adjustments in this regard but take the reported incidence as given.

County-level population data to compute the COVID-19 incidence per 100,000 population are provided by Destatis (2020a). While not part of the main dataset, data collected by Roser et al. (2020) is used in various graphical representations in this study. County-level mobility controls and controls for COVID-19 related restrictions are computed from data provided in daily format by Destatis (2020d) and Infas 360 (2020a).

Descriptive illustrations of the data can be found in the Appendix. Fig. A.1 displays the evolution of the COVID-19 incidence per 100,000 population for each German state, using weekly aggregates of the incidence for the sake of visibility. In each and every state, the daily incidence of COVID-19 was higher by the end of the school breaks than by their start. The three most populous states, Baden-Württemberg, Bavaria and North Rhine-Westphalia, have all seen strong increases in their COVID-19 incidences. Fig. A.2 displays the state-level mobility before and since the start of the school breaks as measured by mobile phone data relative to the previous year, similarly aggregated to the weekly level. The graphs indicate the strongest increases in mobility in the states of Mecklenburg-Western Pomerania, Schleswig-Holstein, and Brandenburg. However, as shown in Fig. A.1, these states had a low COVID-19 incidence at the same time.

6. Results

6.1. Main results

Fig. 4 displays the main results of this study. Each panel shows a plot of the estimated coefficients of the leads and lags and their confidence intervals. The estimates of the final leads and lags are omitted, as their interpretation is not comparable to that of the other estimates due to the binning approach. The horizontal axis indicates the number of days before and since the beginning of the summer school breaks, respectively. The first lead representing the baseline period is omitted. The 0 period hence indicates the day in which the event of the school breaks occurred for the first time. The vertical axis indicates the daily incidence of new COVID-19 cases per 100,000 population in German counties.

Panel 4a shows the graphical results from estimating Eq. (1) only with the various fixed effects but without any controls. Twenty to ten days before the start of the school breaks, the coefficients of the leads are negative. Closer to the start of the school breaks, they then level off closely around zero. Following the start of the school breaks, the first lags remain close to zero or even negative. Only after about 20 days have passed since the start of the school breaks do the estimates of the lags turn clearly positive. The magnitude of the lags then increases steadily till about 42 days of school breaks have passed. Afterwards, the estimates of the lags remain positive but their magnitude appears to decline towards the end of the study period. Note that this specification yields very wide confidence intervals, which render almost every estimate statistically insignificant.

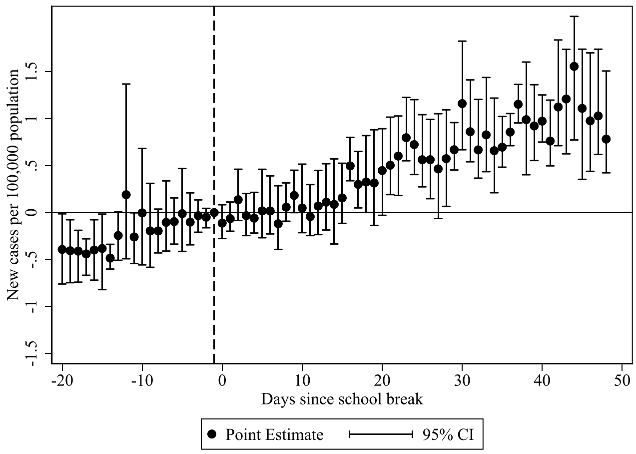
However, the precision of the estimates greatly improves with the addition of the mobility and restriction controls to the regression, as displayed in Panel 4b. Twenty to ten days before the start of the school breaks, most of the negative estimate of the leads are now significantly different from zero. Then, their significance disappears again, as their magnitude shrinks essentially to zero. The first lags after the start of the school breaks remain insignificant, with their magnitudes now even being closer to zero than before. The first positive and significant estimate is observed at sixteen days since the start of the school breaks. Most of the subsequent lags are now significant, too, while they preserve the pattern of increasing magnitude till about 42 days into the school breaks followed by a decline. Interestingly, the mobility and restriction controls also increase the magnitude of the effects estimated for the later days of the school breaks. A potential explanation for this association is that in accordance with the patterns described in Fig. A.2, mobility increased predominantly in states and counties in the north and east of Germany where the COVID-19 incidence remained at an extremely low level throughout the summer months. This explanation is consistent with the modeling results by Klüsener et al. (2020) that there was room for further relaxing NPIs in Germany during the summer months due to the weak dynamics of the pandemic in this period.

Panels 4c and 4d display the results from further adding linear state trends and linear county trends to the regressions, respectively. Each of the additions slightly increases the magnitudes of the lag estimates and increases the precision of the negative lead and the positive lag estimates. Overall, the impact of adding these trends is fairly unremarkable though, suggesting that the results are not driven by potentially underlying linear (or approximately linear) trends in the COVID-19 incidence at the state or county level.

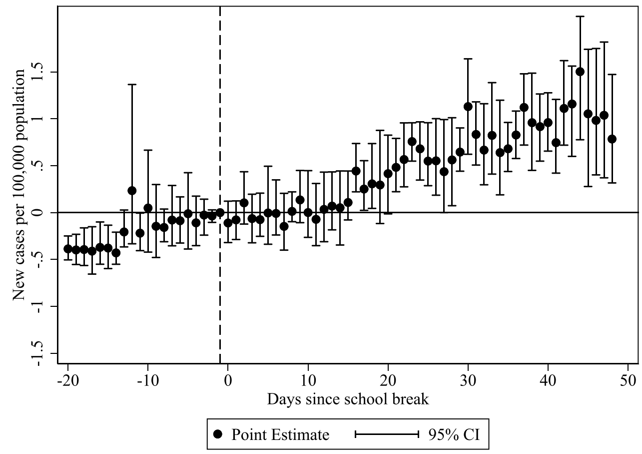
The regression results underlying these plots are reported in Table A.2 in the Appendix. To provide an example of their quantitative interpretation: On the twentieth day since the start of the school breaks, the coefficient estimate of the twentieth lag indicates that on average, the counties on school breaks exhibited 0.321 more new COVID-19 cases per 100,000 population than counties that were not on school breaks yet, with the difference being highly statistically significant.

The negative and significant estimates of the earliest leads warrant a reflection on whether the parallel trends assumption (PTA) might to some extent be violated, which would call the causal interpretation of the positive lag estimates after the start of the school breaks into question. Two observations may alleviate this concern: First, the plateau of the incidence that occurs as the leads move closer to the beginning of the school breaks is retained for a duration of nearly 30 days, which is not immediately compatible with a suspected sustained pre-trend pushing the incidence higher already prior to the beginning of the treatment. Second, if such trends existed, the addition of the linear state- and county-level time trends to the regression could be expected to have a stronger quantitative impact than it actually has.

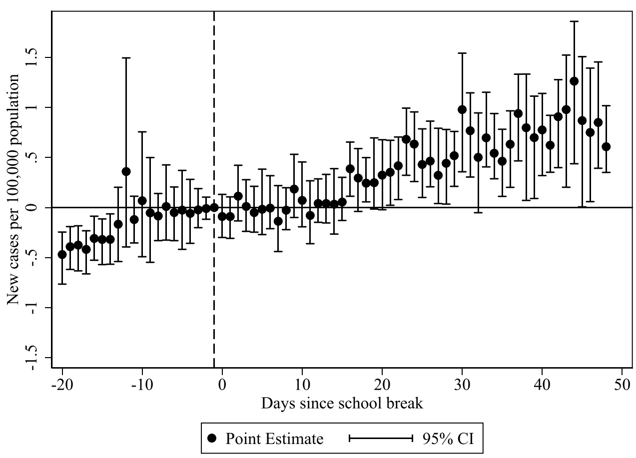
The dynamic pattern of initially small effects that increase substantially in magnitude the longer the states are on school breaks is consistent with international travel movements of residents picking up pace with the start of the school breaks and returning travelers importing infections from abroad upon completion of their trips a few weeks later. The initial zero effects of the school breaks are furthermore consistent with the findings by Isphording et al. (2021) and von Bismarck-Osten



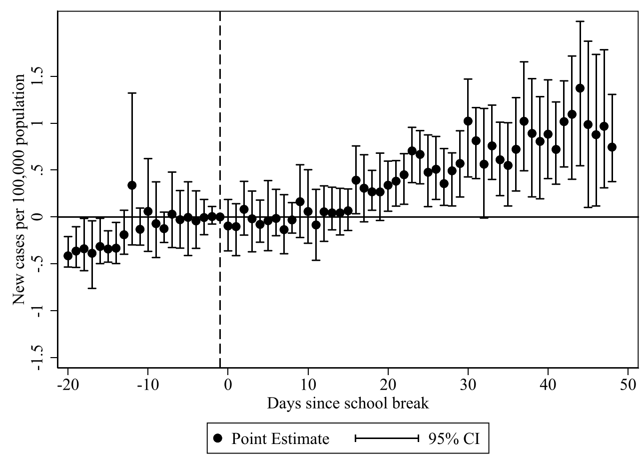
(a) Without city states and with controls



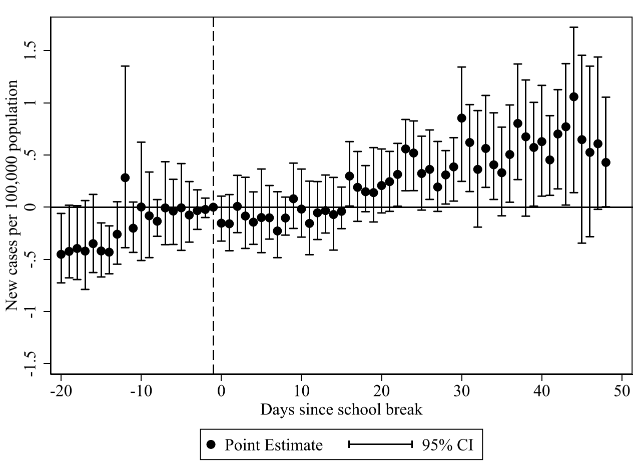
(b) Without city states and with controls and trends



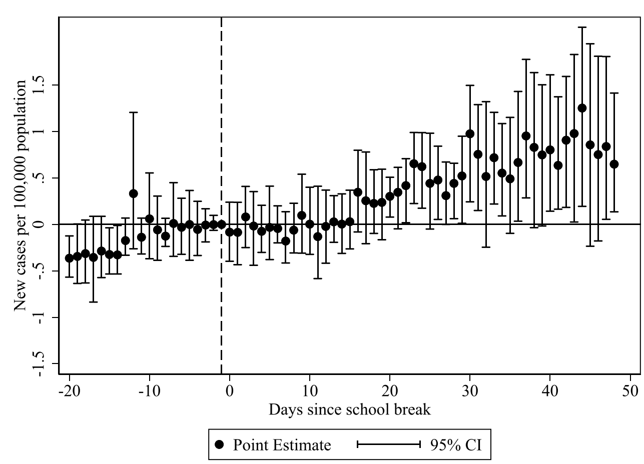
(c) With 1-week lagged controls



(d) With trends and 1-week lagged controls

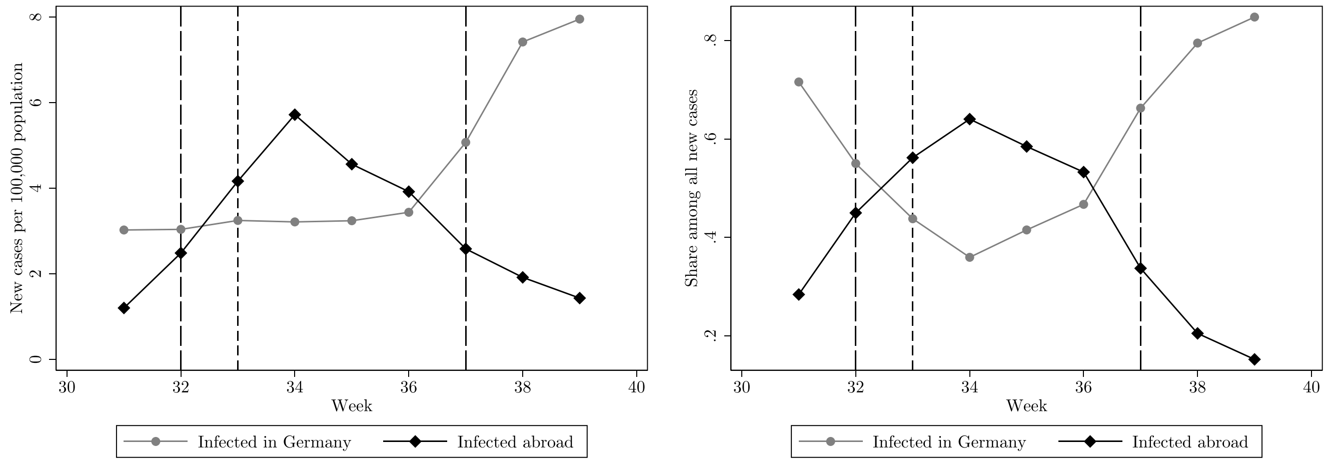


(e) With 2-weeks lagged controls



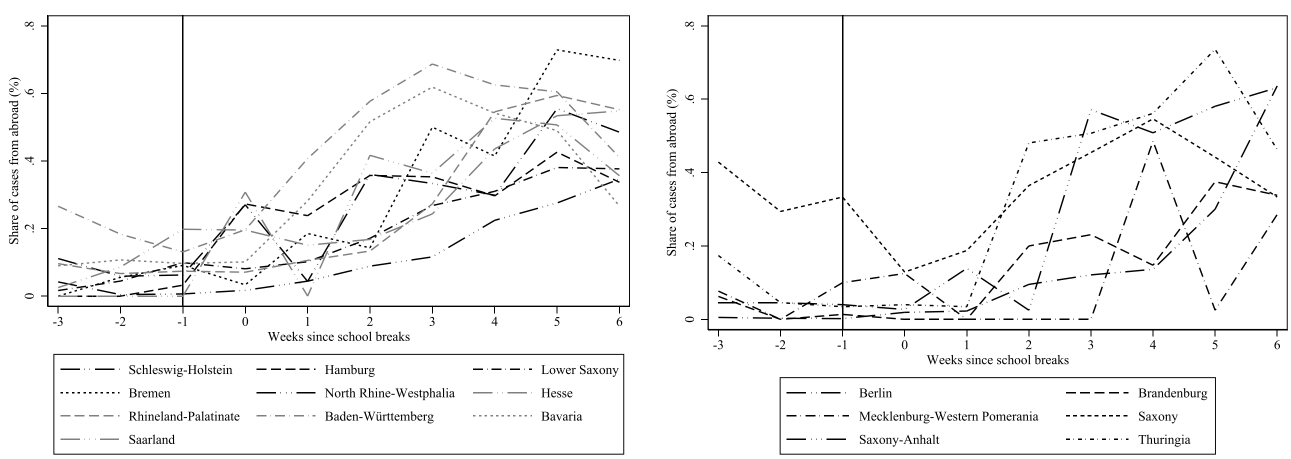
(f) With trends and 2-weeks lagged controls

Fig. 5. Robustness: New cases of COVID-19 per 100k population during school breaks in Germany. Notes: All panels display the daily new confirmed cases of COVID-19 infections in Germany during the summer school breaks in 2020. The effects are estimated by binning all weeks beyond the maximum Leads and Lags before and since the beginning of the school breaks. Standard errors are computed by the wild clustered bootstrap method and clustered at the state level.
Source: Author's own depiction.



(a) New cases of COVID-19 (b) Origins of cases

Fig. 6. Cases of COVID-19 in Germany by places of infection in weeks 30–39 in 2020. Notes: The left panel shows the weekly incidence of COVID-19 per 100,000 population in Germany by likely place of infection during weeks 30–39 in the year 2020. The right panel shows the share of new COVID-19 cases in Germany that each likely place of infection accounts for during weeks 30–39 in the year 2020. The gray lines indicate cases and the share of cases with likely infection in Germany respectively. The black lines indicate cases and the share of cases with likely infection abroad respectively. The vertical long-dashed black lines indicate the first and the last weeks of the school breaks respectively. The vertical short-dashed black lines indicate the start of the mandatory testing regime for travelers returning from declared risk areas.
Source: Author's own depiction based on data provided by RKI (2020a).



(a) Share of new COVID-19 cases from abroad in 10 states (b) Share of new COVID-19 cases from abroad in 6 states

Fig. 7. Share of new cases of COVID-19 acquired abroad during the summer school breaks in 2020. Notes: Both panels show the share of new cases of COVID-19 in Germany with the origin of the infection suspected abroad among all new cases of COVID-19 in Germany. The share in each panel is plotted against the number of weeks prior and since the beginning of the school breaks respectively. The vertical black line in each panel indicates the final week prior to the start of the school breaks.
Source: Author's own depiction based on data provided by RKI (2021a).

et al. (2020) that the school closures have not significantly affected the COVID-19 incidence in Germany. Finally, the fact that the point estimates of the last lags (lags 43–48) are smaller than the point estimates of the earlier lags (lags 35–42) is consistent with the finding of von Bismarck-Osten et al. (2020) that also the school reopenings after the school breaks have not caused a surge in COVID-19 incidence.

6.2. Robustness

Fig. 5 presents a series of robustness checks. Panel 5a displays the coefficients from estimating the baseline regression model with the county-level mobility and restriction controls but omitting the three city states of Berlin, Bremen, and Hamburg from the regression. In Panel 5b, linear county trends are added to the regression excluding the city states. Next, Panels 5c and 5d present results from lagging the mobility and restriction controls by one week in order to allow for potentially delayed effects of the controls on the COVID-19 incidence, with the latter panel also controlling for linear county trends. Finally, Panels 5e and 5f present results from lagging the controls by two weeks. The various modifications primarily affect the precision of the individual estimates but do not change the dynamic pattern. The regression results of the robustness checks are reported in Table A.3 in the Appendix.

6.3. Testing

Recall that free tests for travel returnees were offered since August 1, while the mandatory free testing regime for travel returnees from risk areas was introduced on August 8. Realigning the testing capacities towards the travel returnees has certainly improved the surveillance of imported infections. However, this particular focus raises the question whether the estimates indicating a significant increase in incidence during the later phase of the school breaks are partly driven not only by travelers being at greater risk of infection but also facing a greater chance of being detected as infected upon return than the non-traveling population.

First, there is little reason to suspect that the realignment of testing capacities towards the travel returnees has compromised the detection of cases among the non-traveling population in Germany. A representative seroprevalence survey conducted in the city of Munich estimates that the ratio of undetected infections to confirmed cases (German: *Dunkelziffer*) was less than two during the summer months; a decline from a ratio of four during the first wave in spring (LMU Munich and HelmholtzZentrum Munich, 2020b). While this finding does not necessarily generalize beyond Munich, it corresponds to the very low share of COVID-19 tests that were positive in Germany during the summer months.

Second, the overlap of the school breaks with at least one of the two testing regimes was quite heterogeneous across states: One state's school breaks ended on August 1 (Mecklenburg-Western Pomerania). Two more states concluded their breaks before August 8 (Berlin and Hamburg), while two other states concluded them exactly on August 8 (Brandenburg and Schleswig-Holstein). Four states' school breaks ended within the first week following the start of the mandatory testing regime (Hesse, North Rhine-Westphalia, Rhineland-Palatinate, Saarland). A remainder of five states spent at least the final two weeks of their school breaks under both testing regimes (Bremen, Lower Saxony, Saxony, Saxony-Anhalt, Thuringia). Finally, two states began their school breaks less than one week before the start of the free testing regime and less than two weeks before the start of the mandatory testing regime (Baden-Württemberg and Bavaria).

Third, the introduction of the two testing regimes coincided with a number of factors that are similarly inclined to drive up the COVID-19 incidence among returning travelers: (1) The absolute number of travel returnees was increasing due to the nearing end of the school breaks in several states, which should ceteris paribus increase the number of cases found among travel returnees. (2) The COVID-19 incidence in several important travel destinations was growing, which should ceteris paribus increase the number of cases found among travel returnees. (3) The number of countries designated as risk areas was growing, which increased the scope of the mandatory testing regime, thereby increasing also the potential for 'over'-testing the travel returnees by shifting them from the voluntary to the mandatory regime. (4) The composition of risk areas from which travelers have been returning to Germany has been changing throughout the summer months, whereas it is difficult to assess how the infection risk has changed within the group of risk areas.

The complexity of the testing situation is underlined by Fig. 6. The left panel shows the weekly incidence of COVID-19 in Germany, differentiating between cases with likely place of infection within Germany and cases with likely place of infection abroad. The right panel performs the same differentiation regarding the share of cases that is attributed to each of the two potential locations of infection. Both the incidence and the share of cases from abroad increase markedly with the introduction of the free testing regime in week 32 and the mandatory testing regime in week 33. However, the two variables show considerable variation while both testing regimes were in place, with both variables declining already well before all states have completed their school breaks and the free testing regime has been terminated in week 37.

A reassuring insight can be gained from a state-level disaggregation of the total number of new confirmed cases of COVID-19 per week into cases with likely place of infection abroad and cases with likely place of infection in Germany. The data for this exercise are provided by RKI (2021a). Fig. 7 shows surprisingly similar dynamics in the share of weekly cases from abroad across states over the duration of the school breaks despite states not having been subjected uniformly to the testing regimes, as discussed above. In the later phases of the school breaks of virtually every state, the cases imported from abroad account for a growing and often also predominant share among all new COVID-19 cases. This pattern coincides with the event study estimates presented in Fig. 4 reaching their largest magnitudes near the end of the school breaks, indicating that the estimates are indeed picking new dynamics in COVID-19 cases that originate from the international travel movements.

7. Discussion

First, it is worth pointing out again the context and setting of this study: The restrictions on international travel were loosened during a period of low COVID-19 incidence in Germany, while the COVID-19 incidence in other European countries was fluctuating considerably. This setting facilitates the detection of a rising COVID-19 incidence due to international travel, as it suggests a rather straight-forward mechanism from exposure and infection abroad to detection and potential secondary infections upon return to Germany.

Interpreting the rising incidence in Germany during school breaks as the effect of revived international mobility without being able to distinguish between the traveling and the non-traveling residents would be difficult to justify in most empirical settings. Within the context of the summer months of 2020, the justification of this interpretation rests on (1) the low incidence in Germany prior to and at the beginning of the school breaks, (2) the precise controls for mobility and COVID-19-related restrictions within Germany, and (3) the descriptive associations between the travel movements to countries with higher incidences and the detected cases among infected returnees.

Second, a number of potential sources for bias in the estimated effects can be considered, as suggested by Goodman-Bacon and Marcus (2020): Suppose that, despite the arguments presented in Section 6, the PTA were violated such that the COVID-19 incidence increased not only in the counties already on school breaks but also in the not-yet-treated counties with the beginning of the school breaks. Then the positive and significant lag estimates would actually underestimate the true treatment effects on the counties already on school breaks. In turn, this would render the quantitative interpretation of the lags rather dubious, but it would not affect their qualitative interpretation.

A more worrisome violation of the PTA would occur if COVID-19 cases in the earlier-treated counties were already increasing relative to the later-treated counties prior to the beginning of the school breaks but in a similarly staggered fashion. In this case, the treatment effects would be erroneously interpreted as effects stemming from the school breaks when they actually reflected pre-treatment differences between the counties. While the inspection of the pre-trends is intended to be informative on the likelihood of such a violation of the PTA, Roth (2021) argues that these pre-trend tests might be underpowered and provide poor guidance. As a consequence, Roth (2021) calls for taking context-specific economic knowledge into account when assessing the plausibility of the PTA. This advice, while appropriate, is more difficult to follow in the context of the COVID-19 pandemic, as many factors that govern the dynamics of the recurring waves of infections are still poorly understood. It should therefore be kept in mind that in addition to the general impossibility of proving that the PTA holds, the reliance on pre-trend tests in the context of COVID-19 is not without additional caveats.

The empirical approach of this paper implicitly further assumes that the cases of infected travel returnees are registered in their respective state of residence; with an identical assumption applying to secondary infections caused by travel returnees. While there is little reason to question the first assumption, the second one might be more questionable if infected travel returnees traveled extensively across state borders following their return to Germany. If these domestic movements by infected travel returnees occurred and if they caused significant infections in states other than the respective returnees' state of residence, these spillovers of infections would downward-bias the estimates if the states affected by the spillovers were not on school break yet. If, in turn, they were already on school break, too, but started at a different date, the spillovers would only affect the relative magnitude of the effects. Finally, if they were on school break and if they went on school break at the same time, the spillovers would not bias the estimates, as the spillovers would occur among the group of treated states.

In addition, it is worth noting that Germany received not only an influx of travel returnees with residence in Germany but also an influx of tourists from abroad during the summer months. Some of these tourists might have been infected with SARS-CoV-2, thereby carrying the risk of causing further infections among residents of Germany to which they have been in contact with during their stay. During the four months from June to September, accommodation providers in Germany reported 5.7 million arrivals of guests from abroad (Destatis, 2020e). However, 4.2 million of these guests arrived from neighboring states of Germany where the COVID-19 incidence had been fairly low until close to the end of September. Infections introduced into Germany from abroad by foreign tourists have not been brought up as a concern by the RKI during the same period. Further, the incidence of these potential cases would have to be aligned with the timings of the German school breaks in order to bias the results, for which there is no obvious reason.

Third, recall that the estimates presented in this study should be interpreted as intention-to-treat (ITT) effects, as not every resident in Germany experienced a shock to her/his opportunities to travel from the combination of the relaxed restrictions and the onset of the school breaks - and among those who did, not all have actually traveled abroad during summer. This partial non-compliance dilutes the magnitude of the effects. Further, two important factors affecting the generalizability of the ITT effects are the characteristics of the travelers and the choice of the travel destination countries. According to RKI reports, a large share of infected travelers returned from the Southeast European countries of Kosovo and Bosnia and Herzegovina, which are not popular destinations among German tourists. The RKI situation reports suggest that family visits were the main motivation for travelers from Germany towards these countries. Continuing along this notion, family visits likely involve much closer contact of the travelers to the local population than touristic trips do. Hence, travelers to Southeast European countries not only traveled to countries with a relatively high incidence but also faced a potentially higher risk of infection due to their contact behavior there. If these particular travelers were furthermore more inclined to travel for the purpose of visiting their families than people who would travel only for touristic purposes, then this would render the ITT effects rather specific, as the traveler composition of the summer months would not be representative of the population that had the opportunity to travel. However, the RKI also reported a high number of young infected returning travelers who had traveled for touristic purposes to destinations such as Croatia.

Fourth, this study did not consider whether the loosened travel restrictions may also have affected the incidence of deaths related to COVID-19 in Germany. The death toll from COVID-19 has been very low in Germany during the summer months of 2020, which is explained by the relatively young age of the confirmed cases during the same period. While it cannot be ruled out that the infected returning travelers have experienced a severe, potentially fatal course of the disease, or that infected returning travelers have infected other residents in Germany that have died, the low overall death toll during summer does not warrant a statistical association to the travel patterns. Note further that even the highest levels of COVID-19 incidence observed in other European countries during the summer of 2020 have been dwarfed by the incidences observed during the subsequent autumn and winter waves. Consequently, the risk for a traveler of getting infected while abroad has likely increased, too. The estimates reported in this study are therefore not immediately transferable to other epidemiological contexts.

Finally, the estimates from this study can be used for a simple back-of-the-envelope calculation of the implied total incidence of COVID-19 during the period of the summer breaks. The latter can be calculated by summing up the statistically significant coefficients from one of the models presented in Table A.2 and multiplying the sum by Germany's total population divided by 100,000. The model in column 4 of Table A.2, which controls for county-level mobility and restrictions, as well as for county-level linear time trends, yields an upper limit of 18,460 cases. It could be expected that the total incidence implied by the estimates is somewhat lower than the approx. 23,000 total confirmed cases among travel returnees from the same period due to the ITT character of the estimates. However, given that the estimates presented here would also reflect potential secondary infections caused by travel returnees, this back-of-the-envelope calculation may suggest that the surveillance of travel returnees has been adequate in terms of limiting the risk of creating sustained epidemic dynamics from the imported infections. The 18,460 cases implied by the reduced-form effects can further account for 26.3% of all cases of COVID-19 that have been confirmed in Germany between the earliest starting date and the latest conclusion date of the school breaks. Statistics on passenger arrivals in Germany by plane in the period from June to September 2020 can be used to compute an upper bound on the infection rate of arriving travelers: If all 18,460 cases implied by the reduced-form effects originated from arrivals by plane, which is unlikely, this would imply an infection rate of 0.216% among the approx. 8.5 million air passengers that have arrived in Germany during this period (Destatis, 2020c).

8. Conclusions

International travel represents an important element of modern lifestyle and a cornerstone to many tourism-oriented economies across the globe. During a pandemic, however, the benefits of international travel, such as the consumer expenditures flowing into the tourism industry, the leisure time enjoyed during vacations, and the important non-pecuniary benefit of visiting family members who live abroad, should be balanced against the public health risks from international mobility.

This study provides evidence that partially reviving international travel during the summer months of 2020 led to a significant increase in the COVID-19 incidence in Germany. In this context, travel took place between a country with a then low incidence, Germany, and several other European countries with higher and more volatile incidence rates. To mitigate the risks from international travel, Germany implemented first a voluntary testing regime for travel returnees and then shortly after a mandatory testing regime for travelers returning from designated risk areas. While this study cannot fully disentangle the effects of the two testing regimes on the incidence among travel returnees from other important time-varying epidemiological factors, it does not find evidence that the public health surveillance has missed a significant number of imported infections or that imported infections have caused a significant number of secondary infections within Germany.

Importantly, several characteristics of this study's setting do not unconditionally carry over to other contexts of international travel during the COVID-19 pandemic. New, more contagious variants of SARS-CoV-2 are suited to make infections among travelers more likely to occur and to increase the risk of importing these infections upon return. Further, significant PCR testing capacities could be realigned towards the travel returnees during the summer months of 2020 only because the incidence and hence the demand for tests were low within Germany at the same time. Other periods may not allow such a shift but require a significant expansion of PCR testing capacities overall. Finally, vaccines against SARS-CoV-2 were still unavailable in the summer of 2020; it remains to be investigated how the rising vaccination rates in the course of the year 2021 affect the infection and transmission risks of travelers.

Acknowledgments

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Appendices

See: Tables A.1, A.2, and A.3.

Table A.1
Share of family members among the population by German states.

State	Share of family members among population
Schleswig-Holstein	0.3464
Hamburg	0.3627
Lower Saxony	0.3569
Bremen	0.3347
North Rhine-Westphalia	0.3627
Hesse	0.3680
Rhineland-Palatinate	0.3576
Baden-Württemberg	0.3715
Bavaria	0.3626
Saarland	0.3253
Berlin	0.3499
Brandenburg	0.3418
Mecklenburg-Western Pomerania	0.3265
Saxony	0.3374
Saxony-Anhalt	0.3144
Thuringia	0.3262

Notes: The table reports the share of family members among the population in each of Germany's states in 2019. Only families with children younger than 18 years are counted.
Source: Author's own calculation based on data provided by the Federal Statistical Office.

Table A.2
Main estimates.

	(1)	(2)	(3)	(4)
	New cases of COVID-19 per 100,000 population			
Lead21	-0.431 * **	-0.601 * **	-0.530 * **	-0.539 * **
	(0.101)	(0.102)	(0.103)	(0.099)
Lead20	-0.353 * **	-0.425 * **	-0.420 * **	-0.433 * **
	(0.134)	(0.133)	(0.133)	(0.128)
Lead19	-0.315 * *	-0.386 * **	-0.377 * **	-0.392 * **
	(0.133)	(0.133)	(0.132)	(0.127)
Lead18	-0.281 * *	-0.366 * **	-0.353 * **	-0.360 * **
	(0.130)	(0.130)	(0.129)	(0.124)
Lead17	-0.328 * *	-0.419 * **	-0.397 * **	-0.406 * **
	(0.133)	(0.133)	(0.133)	(0.128)
Lead16	-0.256 *	-0.345 * **	-0.329 * *	-0.336 * **
	(0.133)	(0.133)	(0.132)	(0.127)
Lead15	-0.296 * *	-0.345 * **	-0.339 * **	-0.357 * **
	(0.123)	(0.122)	(0.122)	(0.117)
Lead14	-0.291 * *	-0.413 * **	-0.355 * **	-0.376 * **
	(0.132)	(0.132)	(0.132)	(0.127)
Lead13	-0.154	-0.271 * *	-0.231 *	-0.243 *
	(0.134)	(0.133)	(0.133)	(0.128)
Lead12	0.363 * **	0.234 *	0.272 * *	0.268 * *
	(0.130)	(0.130)	(0.129)	(0.124)
Lead11	-0.098	-0.212	-0.170	-0.184
	(0.130)	(0.129)	(0.129)	(0.124)
Lead10	0.095	-0.018	0.024	0.014
	(0.134)	(0.133)	(0.132)	(0.127)
Lead9	-0.031	-0.141	-0.100	-0.110
	(0.133)	(0.133)	(0.132)	(0.127)
Lead8	-0.106	-0.194	-0.163	-0.176
	(0.120)	(0.120)	(0.119)	(0.115)

(continued on next page)

Table A.2 (*continued*)

	(1)	(2)	(3)	(4)
Lead7	0.042	-0.015	0.008	-0.000
	(0.132)	(0.132)	(0.131)	(0.126)
Lead6	-0.015	-0.056	-0.049	-0.057
	(0.134)	(0.133)	(0.132)	(0.127)
Lead5	0.014	-0.013	-0.020	-0.029
	(0.130)	(0.129)	(0.128)	(0.123)
Lead4	-0.032	-0.055	-0.066	-0.075
	(0.130)	(0.129)	(0.128)	(0.123)
Lead3	0.008	-0.013	-0.020	-0.029
	(0.134)	(0.133)	(0.132)	(0.127)
Lead2	0.017	-0.016	-0.022	-0.023
	(0.133)	(0.132)	(0.132)	(0.127)
Lag0	-0.113	-0.107	-0.106	-0.099
	(0.133)	(0.132)	(0.131)	(0.126)
Lag1	-0.122	-0.101	-0.123	-0.111
	(0.135)	(0.134)	(0.133)	(0.128)
Lag2	0.060	0.117	0.082	0.090
	(0.131)	(0.130)	(0.129)	(0.124)
Lag3	-0.045	0.004	-0.033	-0.017
	(0.130)	(0.129)	(0.128)	(0.123)
Lag4	-0.104	-0.063	-0.092	-0.073
	(0.135)	(0.134)	(0.133)	(0.128)
Lag5	-0.062	-0.021	-0.017	-0.010
	(0.134)	(0.133)	(0.133)	(0.128)
Lag6	-0.088	-0.030	-0.055	-0.030
	(0.120)	(0.120)	(0.119)	(0.115)
Lag7	-0.223 *	-0.142	-0.154	-0.140
	(0.134)	(0.133)	(0.133)	(0.128)
Lag8	-0.117	-0.024	-0.051	-0.034
	(0.136)	(0.136)	(0.135)	(0.130)
Lag9	0.054	0.163	0.135	0.148
	(0.131)	(0.131)	(0.130)	(0.125)
Lag10	-0.046	0.064	0.038	0.054
	(0.131)	(0.131)	(0.130)	(0.125)
Lag11	-0.187	-0.085	-0.104	-0.084
	(0.136)	(0.136)	(0.136)	(0.130)
Lag12	-0.071	0.041	0.023	0.036
	(0.135)	(0.135)	(0.135)	(0.129)
Lag13	-0.087	0.036	0.006	0.029
	(0.123)	(0.123)	(0.123)	(0.118)
Lag14	-0.119	0.016	-0.005	0.019
	(0.136)	(0.136)	(0.135)	(0.130)
Lag15	-0.099	0.048	0.017	0.041
	(0.137)	(0.138)	(0.137)	(0.132)
Lag16	0.211	0.383 * **	0.354 * **	0.365 * **
	(0.132)	(0.132)	(0.132)	(0.127)
Lag17	0.121	0.286 * *	0.256 *	0.278 * *
	(0.135)	(0.135)	(0.135)	(0.130)
Lag18	0.082	0.233 *	0.216	0.244 *
	(0.138)	(0.138)	(0.138)	(0.133)
Lag19	0.075	0.225	0.208	0.236 *
	(0.137)	(0.137)	(0.137)	(0.132)
Lag20	0.141	0.321 * **	0.297 * *	0.321 * **
	(0.123)	(0.124)	(0.124)	(0.119)
Lag21	0.155	0.347 * *	0.329 * *	0.358 * **
	(0.137)	(0.138)	(0.138)	(0.133)
Lag22	0.220	0.424 * **	0.398 * **	0.426 * **
	(0.139)	(0.140)	(0.139)	(0.134)
Lag23	0.452 * **	0.670 * **	0.642 * **	0.661 * **
	(0.134)	(0.135)	(0.135)	(0.130)
Lag24	0.420 * **	0.645 * **	0.619 * **	0.638 * **
	(0.136)	(0.137)	(0.137)	(0.132)
Lag25	0.232 *	0.438 * **	0.422 * **	0.452 * **
	(0.139)	(0.141)	(0.140)	(0.135)
Lag26	0.265 *	0.452 * **	0.445 * **	0.470 * **
	(0.138)	(0.140)	(0.139)	(0.134)
Lag27	0.093	0.321 * *	0.335 * **	0.347 * **
	(0.126)	(0.128)	(0.128)	(0.123)
Lag28	0.203	0.431 * **	0.447 * **	0.473 * **
	(0.139)	(0.141)	(0.141)	(0.135)
Lag29	0.275 * *	0.513 * **	0.522 * **	0.548 * **
	(0.140)	(0.142)	(0.142)	(0.137)
Lag30	0.715 * **	0.979 * **	0.992 * **	1.004 * **
	(0.135)	(0.138)	(0.138)	(0.132)
Lag31	0.502 * **	0.759 * **	0.770 * **	0.792 * **

(*continued on next page*)

Table A.2 (*continued*)

	(1)	(2)	(3)	(4)
	(0.138)	(0.140)	(0.140)	(0.135)
Lag32	0.256 *	0.498 * **	0.521 * **	0.549 * **
	(0.140)	(0.143)	(0.143)	(0.138)
Lag33	0.462 * **	0.708 * **	0.729 * **	0.755 * **
	(0.139)	(0.143)	(0.143)	(0.137)
Lag34	0.281 * *	0.554 * **	0.574 * **	0.591 * **
	(0.130)	(0.134)	(0.134)	(0.129)
Lag35	0.205	0.501 * **	0.528 * **	0.543 * **
	(0.141)	(0.144)	(0.144)	(0.139)
Lag36	0.372 * **	0.672 * **	0.690 * **	0.704 * **
	(0.142)	(0.145)	(0.146)	(0.140)
Lag37	0.649 * **	0.971 * **	0.987 * **	0.995 * **
	(0.139)	(0.143)	(0.144)	(0.138)
Lag38	0.519 * **	0.846 * **	0.865 * **	0.874 * **
	(0.141)	(0.145)	(0.146)	(0.140)
Lag39	0.437 * **	0.754 * **	0.787 * **	0.798 * **
	(0.142)	(0.146)	(0.147)	(0.141)
Lag40	0.501 * **	0.838 * **	0.880 * **	0.872 * **
	(0.141)	(0.146)	(0.146)	(0.141)
Lag41	0.322 * *	0.620 * **	0.675 * **	0.680 * **
	(0.134)	(0.138)	(0.139)	(0.134)
Lag42	0.585 * **	0.918 * **	0.990 * **	0.978 * **
	(0.143)	(0.147)	(0.148)	(0.142)
Lag43	0.649 * **	1.012 * **	1.068 * **	1.039 * **
	(0.143)	(0.148)	(0.150)	(0.144)
Lag44	0.911 * **	1.290 * **	1.349 * **	1.312 * **
	(0.142)	(0.147)	(0.148)	(0.143)
Lag45	0.517 * **	0.907 * **	0.973 * **	0.929 * **
	(0.144)	(0.149)	(0.150)	(0.145)
Lag46	0.410 * **	0.809 * **	0.923 * **	0.880 * **
	(0.144)	(0.151)	(0.153)	(0.147)
Lag47	0.486 * **	0.877 * **	0.989 * **	0.948 * **
	(0.143)	(0.151)	(0.153)	(0.147)
Lag48	0.281 * *	0.682 * **	0.788 * **	0.751 * **
	(0.137)	(0.145)	(0.147)	(0.141)
Lag49	-0.173	0.270 * *	0.433 * **	0.397 * **
	(0.121)	(0.131)	(0.135)	(0.130)
Observations	50,526	50,526	50,526	50,526
County FE	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes
Day-of-week × State FE	Yes	Yes	Yes	Yes
Linear state trends	No	No	Yes	No
Linear county trends	No	No	No	Yes
Mobility controls	No	Yes	Yes	Yes
Restriction controls	No	Yes	Yes	Yes

Notes: The table reports event-study estimates of the effect of the summer school breaks on the daily COVID-19 incidence per 100,000 population in Germany in 2020. All regression models were estimated by binning days beyond the maximum included number of Leads and Lags. The Lead estimates indicate days prior to the beginning of the school breaks. The last day before the beginning of the school breaks is omitted (Lead1). The Lag estimates indicate days since the beginning of the school breaks. Column 1 reports results from running the regression without covariates, including only the county and day FE, as well as the interactions of the day-of-the-week and state FE. Column 2 reports results from adding the mobility and restriction controls to the regression. Column 3 reports results from adding state-level linear trends to the regression. Column 4 reports results from adding county-level linear trends to the regression. Bootstrapped standard errors clustered at the state level reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, * $p < 0.1$.
Source: Author's own analysis.

Table A.3
Robustness checks.

	(1)	(2)	(3)	(4)	(5)	(6)
			New cases of COVID-19 per 100,000 population			
Lead21	-0.660 * **	-0.548 * **	-0.571 * **	-0.488 * **	-0.574 * **	-0.470 * **
	(0.107)	(0.104)	(0.102)	(0.098)	(0.101)	(0.098)
Lead20	-0.393 * **	-0.387 * **	-0.470 * **	-0.415 * **	-0.452 * **	-0.363 * **
	(0.140)	(0.135)	(0.133)	(0.128)	(0.133)	(0.127)
Lead19	-0.407 * **	-0.398 * **	-0.392 * **	-0.364 * **	-0.424 * **	-0.344 * **
	(0.139)	(0.134)	(0.132)	(0.127)	(0.132)	(0.126)
Lead18	-0.412 * **	-0.394 * **	-0.375 * **	-0.340 * **	-0.395 * **	-0.314 * *
	(0.135)	(0.130)	(0.129)	(0.124)	(0.129)	(0.124)
Lead17	-0.441 * **	-0.411 * **	-0.419 * **	-0.388 * **	-0.421 * **	-0.355 * **
	(0.140)	(0.134)	(0.133)	(0.127)	(0.133)	(0.127)
Lead16	-0.400 * **	-0.371 * **	-0.309 * *	-0.315 * *	-0.349 * **	-0.286 * *
	(0.140)	(0.134)	(0.133)	(0.127)	(0.133)	(0.127)
Lead15	-0.383 * **	-0.378 * **	-0.319 * **	-0.342 * **	-0.420 * **	-0.324 * **
	(0.127)	(0.122)	(0.122)	(0.117)	(0.122)	(0.117)
Lead14	-0.486 * **	-0.429 * **	-0.319 * *	-0.333 * **	-0.431 * **	-0.328 * **
	(0.138)	(0.133)	(0.132)	(0.126)	(0.132)	(0.126)
Lead13	-0.245 *	-0.206	-0.167	-0.190	-0.259 *	-0.173
	(0.140)	(0.134)	(0.133)	(0.128)	(0.133)	(0.127)
Lead12	0.190	0.234 *	0.360 * **	0.338 * **	0.283 * *	0.332 * **
	(0.137)	(0.131)	(0.129)	(0.124)	(0.129)	(0.124)
Lead11	-0.260 *	-0.219 *	-0.120	-0.132	-0.201	-0.138
	(0.135)	(0.129)	(0.129)	(0.124)	(0.129)	(0.124)
Lead10	-0.003	0.049	0.068	0.059	0.002	0.060
	(0.140)	(0.134)	(0.133)	(0.127)	(0.133)	(0.127)
Lead9	-0.195	-0.146	-0.053	-0.072	-0.082	-0.058
	(0.139)	(0.134)	(0.133)	(0.127)	(0.132)	(0.127)
Lead8	-0.195	-0.158	-0.085	-0.125	-0.135	-0.126
	(0.124)	(0.119)	(0.120)	(0.114)	(0.120)	(0.114)
Lead7	-0.105	-0.079	0.010	0.029	-0.007	0.011
	(0.138)	(0.133)	(0.132)	(0.126)	(0.132)	(0.126)
Lead6	-0.097	-0.086	-0.050	-0.030	-0.037	-0.031
	(0.140)	(0.134)	(0.133)	(0.127)	(0.133)	(0.127)
Lead5	-0.011	-0.012	-0.026	-0.005	-0.006	-0.000
	(0.136)	(0.130)	(0.129)	(0.123)	(0.129)	(0.123)
Lead4	-0.102	-0.109	-0.058	-0.043	-0.075	-0.054
	(0.135)	(0.129)	(0.129)	(0.123)	(0.129)	(0.123)
Lead3	-0.033	-0.026	-0.023	-0.007	-0.033	-0.007
	(0.140)	(0.134)	(0.133)	(0.127)	(0.133)	(0.127)
Lead2	-0.050	-0.036	-0.010	0.004	-0.022	-0.002
	(0.139)	(0.133)	(0.132)	(0.127)	(0.133)	(0.127)
Lag0	-0.113	-0.110	-0.090	-0.097	-0.153	-0.083
	(0.139)	(0.133)	(0.132)	(0.126)	(0.132)	(0.126)
Lag1	-0.065	-0.078	-0.090	-0.103	-0.159	-0.085
	(0.141)	(0.135)	(0.134)	(0.128)	(0.134)	(0.128)
Lag2	0.137	0.104	0.114	0.081	0.008	0.081
	(0.136)	(0.130)	(0.130)	(0.124)	(0.130)	(0.124)
Lag3	-0.033	-0.064	0.011	-0.020	-0.085	-0.017
	(0.136)	(0.131)	(0.129)	(0.123)	(0.129)	(0.123)
Lag4	-0.060	-0.075	-0.050	-0.079	-0.144	-0.074
	(0.141)	(0.135)	(0.134)	(0.128)	(0.134)	(0.128)
Lag5	0.018	-0.004	-0.017	-0.040	-0.099	-0.032
	(0.140)	(0.134)	(0.133)	(0.128)	(0.133)	(0.128)
Lag6	0.017	-0.009	-0.006	-0.016	-0.102	-0.044
	(0.125)	(0.119)	(0.120)	(0.115)	(0.120)	(0.115)
Lag7	-0.119	-0.147	-0.137	-0.136	-0.227 *	-0.179
	(0.141)	(0.135)	(0.134)	(0.128)	(0.133)	(0.128)
Lag8	0.057	0.013	-0.026	-0.032	-0.104	-0.061
	(0.143)	(0.137)	(0.136)	(0.130)	(0.135)	(0.129)
Lag9	0.182	0.135	0.185	0.162	0.081	0.097
	(0.137)	(0.131)	(0.131)	(0.125)	(0.130)	(0.125)
Lag10	0.049	0.001	0.072	0.058	-0.019	0.003
	(0.138)	(0.132)	(0.131)	(0.126)	(0.131)	(0.125)
Lag11	-0.043	-0.072	-0.079	-0.085	-0.155	-0.131
	(0.143)	(0.137)	(0.136)	(0.131)	(0.136)	(0.130)
Lag12	0.069	0.035	0.041	0.055	-0.054	-0.021
	(0.142)	(0.136)	(0.135)	(0.130)	(0.135)	(0.129)
Lag13	0.110	0.069	0.040	0.043	-0.033	0.028
	(0.128)	(0.123)	(0.123)	(0.118)	(0.123)	(0.117)
Lag14	0.087	0.052	0.033	0.043	-0.071	0.004
	(0.143)	(0.137)	(0.136)	(0.130)	(0.135)	(0.130)
Lag15	0.156	0.108	0.055	0.066	-0.039	0.029
	(0.145)	(0.139)	(0.138)	(0.132)	(0.137)	(0.131)
Lag16	0.497 * **	0.444 * **	0.388 * **	0.392 * **	0.298 * *	0.347 * **

(*continued on next page*)

Table A.3 (*continued*)

	(1)	(2)	(3)	(4)	(5)	(6)
	(0.138)	(0.132)	(0.132)	(0.127)	(0.132)	(0.126)
Lag17	0.300 * *	0.251 *	0.295 * *	0.306 * *	0.191	0.256 * *
	(0.142)	(0.136)	(0.135)	(0.130)	(0.135)	(0.129)
Lag18	0.326 * *	0.307 * *	0.244 *	0.268 * *	0.148	0.227 *
	(0.146)	(0.140)	(0.138)	(0.133)	(0.138)	(0.132)
Lag19	0.314 * *	0.295 * *	0.249 *	0.267 * *	0.139	0.238 *
	(0.144)	(0.139)	(0.137)	(0.132)	(0.137)	(0.131)
Lag20	0.448 * **	0.415 * **	0.324 * **	0.339 * **	0.207 *	0.303 * *
	(0.130)	(0.125)	(0.124)	(0.119)	(0.124)	(0.119)
Lag21	0.504 * **	0.483 * **	0.352 * *	0.381 * **	0.244 *	0.346 * **
	(0.146)	(0.140)	(0.138)	(0.133)	(0.138)	(0.132)
Lag22	0.602 * **	0.566 * **	0.418 * **	0.450 * **	0.314 * *	0.418 * **
	(0.147)	(0.141)	(0.140)	(0.134)	(0.139)	(0.133)
Lag23	0.798 * **	0.758 * **	0.682 * **	0.704 * **	0.559 * **	0.654 * **
	(0.141)	(0.136)	(0.135)	(0.130)	(0.135)	(0.129)
Lag24	0.725 * **	0.680 * **	0.635 * **	0.667 * **	0.519 * **	0.623 * **
	(0.145)	(0.139)	(0.138)	(0.132)	(0.137)	(0.131)
Lag25	0.563 * **	0.550 * **	0.431 * **	0.476 * **	0.323 * *	0.442 * **
	(0.148)	(0.142)	(0.140)	(0.135)	(0.140)	(0.134)
Lag26	0.563 * **	0.552 * **	0.465 * **	0.509 * **	0.362 * **	0.479 * **
	(0.147)	(0.141)	(0.139)	(0.134)	(0.139)	(0.133)
Lag27	0.465 * **	0.437 * **	0.322 * *	0.355 * **	0.194	0.311 * *
	(0.134)	(0.129)	(0.128)	(0.123)	(0.127)	(0.122)
Lag28	0.574 * **	0.562 * **	0.443 * **	0.492 * **	0.309 * *	0.442 * **
	(0.148)	(0.143)	(0.141)	(0.135)	(0.140)	(0.135)
Lag29	0.670 * **	0.646 * **	0.519 * **	0.570 * **	0.386 * **	0.523 * **
	(0.150)	(0.144)	(0.142)	(0.136)	(0.141)	(0.136)
Lag30	1.162 * **	1.131 * **	0.980 * **	1.022 * **	0.854 * **	0.976 * **
	(0.143)	(0.138)	(0.137)	(0.132)	(0.136)	(0.131)
Lag31	0.862 * **	0.834 * **	0.768 * **	0.813 * **	0.620 * **	0.755 * **
	(0.148)	(0.143)	(0.140)	(0.134)	(0.139)	(0.134)
Lag32	0.668 * **	0.667 * **	0.502 * **	0.562 * **	0.362 * *	0.517 * **
	(0.151)	(0.145)	(0.143)	(0.137)	(0.142)	(0.137)
Lag33	0.828 * **	0.823 * **	0.698 * **	0.758 * **	0.563 * **	0.719 * **
	(0.149)	(0.144)	(0.142)	(0.136)	(0.141)	(0.136)
Lag34	0.659 * **	0.640 * **	0.543 * **	0.610 * **	0.406 * **	0.553 * **
	(0.140)	(0.136)	(0.133)	(0.128)	(0.132)	(0.127)
Lag35	0.697 * **	0.681 * **	0.463 * **	0.550 * **	0.330 * *	0.492 * **
	(0.151)	(0.146)	(0.143)	(0.138)	(0.142)	(0.137)
Lag36	0.858 * **	0.828 * **	0.633 * **	0.721 * **	0.505 * **	0.668 * **
	(0.153)	(0.148)	(0.144)	(0.139)	(0.144)	(0.138)
Lag37	1.154 * **	1.122 * **	0.940 * **	1.021 * **	0.803 * **	0.953 * **
	(0.149)	(0.145)	(0.142)	(0.137)	(0.141)	(0.136)
Lag38	0.990 * **	0.959 * **	0.799 * **	0.891 * **	0.675 * **	0.830 * **
	(0.153)	(0.148)	(0.144)	(0.139)	(0.143)	(0.138)
Lag39	0.923 * **	0.917 * **	0.700 * **	0.806 * **	0.572 * **	0.749 * **
	(0.154)	(0.149)	(0.145)	(0.140)	(0.144)	(0.140)
Lag40	0.975 * **	0.960 * **	0.776 * **	0.883 * **	0.628 * **	0.803 * **
	(0.152)	(0.148)	(0.145)	(0.140)	(0.144)	(0.139)
Lag41	0.762 * **	0.746 * **	0.624 * **	0.720 * **	0.453 * **	0.636 * **
	(0.145)	(0.141)	(0.138)	(0.133)	(0.136)	(0.131)
Lag42	1.125 * **	1.111 * **	0.909 * **	1.017 * **	0.701 * **	0.907 * **
	(0.154)	(0.150)	(0.146)	(0.141)	(0.145)	(0.140)
Lag43	1.211 * **	1.158 * **	0.980 * **	1.095 * **	0.771 * **	0.979 * **
	(0.156)	(0.152)	(0.147)	(0.143)	(0.146)	(0.141)
Lag44	1.558 * **	1.503 * **	1.264 * **	1.374 * **	1.058 * **	1.253 * **
	(0.154)	(0.150)	(0.146)	(0.141)	(0.144)	(0.139)
Lag45	1.111 * **	1.053 * **	0.870 * **	0.986 * **	0.647 * **	0.857 * **
	(0.157)	(0.153)	(0.148)	(0.143)	(0.146)	(0.141)
Lag46	0.978 * **	0.983 * **	0.751 * **	0.877 * **	0.525 * **	0.752 * **
	(0.159)	(0.156)	(0.148)	(0.144)	(0.147)	(0.142)
Lag47	1.030 * **	1.038 * **	0.851 * **	0.967 * **	0.608 * **	0.839 * **
	(0.158)	(0.155)	(0.147)	(0.143)	(0.146)	(0.142)
Lag48	0.783 * **	0.785 * **	0.609 * **	0.745 * **	0.429 * **	0.649 * **
	(0.152)	(0.150)	(0.141)	(0.137)	(0.140)	(0.135)
Lag49	0.498 * **	0.588 * **	0.234 *	0.454 * **	0.029	0.350 * **
	(0.137)	(0.138)	(0.129)	(0.128)	(0.125)	(0.123)
Observations	50,022	50,022	50,526	50,526	50,526	50,526
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-week × State FE	Yes	Yes	Yes	Yes	Yes	Yes
Linear county trends	No	Yes	No	Yes	No	Yes
Mobility controls	Yes	Yes	No	No	No	No
Restriction controls	No	No	No	No	No	No
Lagged mobility controls	No	No	Yes	Yes	Yes	Yes

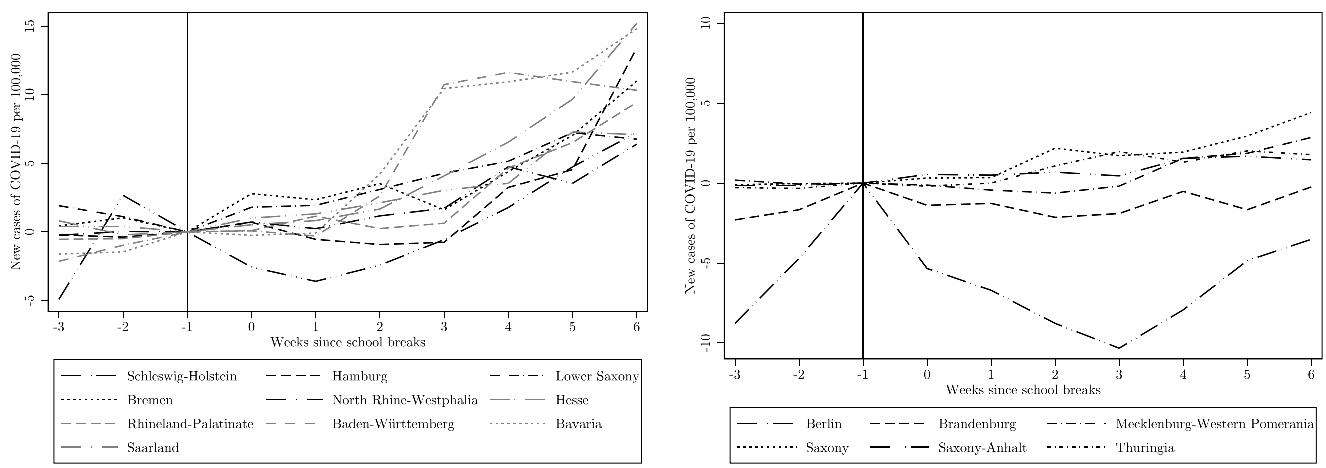
(*continued on next page*)

Table A.3 (*continued*)

	(1)	(2)	(3)	(4)	(5)	(6)
Lagged restriction controls	No	No	Yes	Yes	Yes	Yes
City states	No	No	Yes	Yes	Yes	Yes

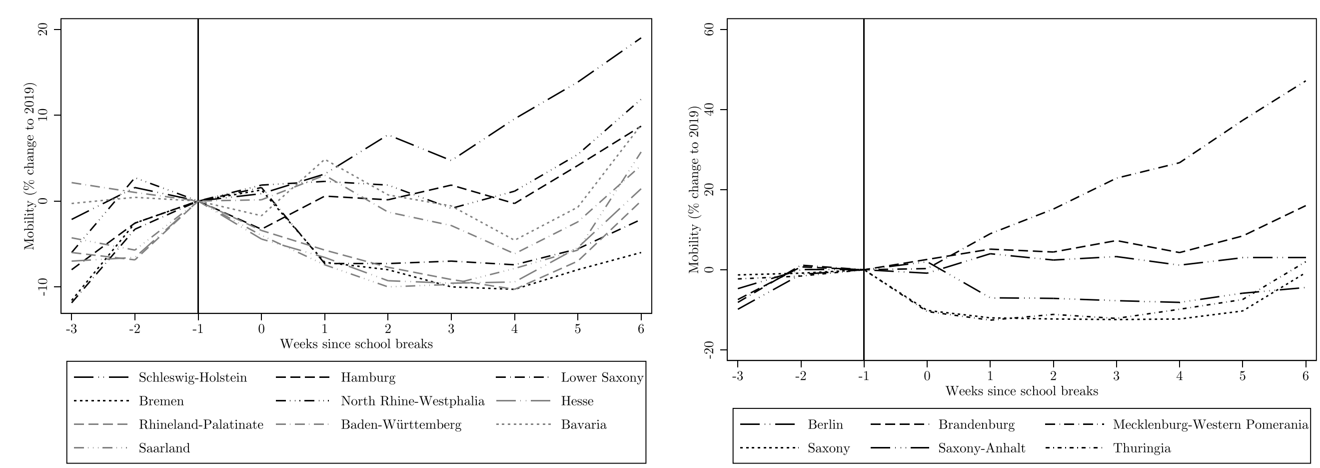
Notes: The table reports robustness checks of event-study estimates of the effect of the summer school breaks on the daily COVID-19 incidence per 100,000 population in Germany in 2020. All regression models were estimated by binning days beyond the maximum included number of Leads and Lags. The Lead estimates indicate days prior to the beginning of the school breaks. The last day before the beginning of the school breaks is omitted (Lead1). The Lag estimates indicate days since the beginning of the school breaks. Columns 1 and 2 report results from omitting the three German city states from the regression, with column 2 additionally controlling for county-level linear trends. Columns 3 and 4 report results from adding the mobility and restriction controls lagged by seven days to the regressions, with column 4 additionally controlling for county-level linear trends. Column 5 and 6 report results from adding the mobility and restriction controls lagged by 14 days to the regressions, with column 6 additionally controlling for county-level linear trends. Bootstrapped standard errors clustered at the state level reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.
Source: Author's own analysis.

See: Figs. A.1, A.2.



(a) COVID-19 in 10 states around the school breaks (b) COVID-19 in 6 states around the school breaks

Fig. A.1. New cases of COVID-19 by state around the school breaks. Notes: Both panels display the evolution of new confirmed cases of COVID-19 per 100,000 population in each of Germany's 16 states during the summer school breaks in 2020. New cases are displayed up to three weeks before and six weeks since the start of the school breaks in each state. The last period before the beginning of the school breaks is used as the reference period.
Source: Author's own depiction based on data provided by RKI (2020c); Destatis (2020a).



(a) Mobility trends in 10 states around the school breaks (b) Mobility trends in 6 states around the school breaks

Fig. A.2. Mobility trends in Germany around the school breaks. Notes: Both panels display the evolution of mobility in each of Germany's 16 states before and after the beginning of the summer school breaks in 2020. Mobility changes are displayed up to three weeks before and six weeks since the start of the school breaks in each state. The last period before the beginning of the school breaks is used as the reference period.
Source: Author's own depiction based on data provided by Destatis (2020d).

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